

XXVII. *An Attempt to compare and connect the Thermometer for strong Fire, described in Vol. LXXII. of the Philosophical Transactions, with the common Mercurial Ones. By Mr. Josiah Wedgwood, F. R. S. Potter to Her Majesty.*

Read May 13, 1784.

THIS thermometer, which I had the honour of laying before the Royal Society in May 1782, has now been found, from extensive experience, both in my manufactories and experimental enquiries, to answer the expectations I had conceived of it as a measure of all degrees of common fire above ignition: but at present it stands in a detached state, not connected with any other, as it does not begin to take place till the heat is too great to be measured or supported by mercurial ones.

What is now therefore wanting, to give us clear ideas of the value of its degrees, is, to connect it with one which long use has rendered familiar to us; so that if the scale of the common thermometer be continued indefinitely upwards as a standard, the divisions of mine may be reduced to that scale, and we may thus have the whole range of the degrees of heat brought into one uniform series, expressed in one language, and comparable in every part, from the lowest that have hitherto been produced by any artificial freezing mixtures, up to the highest that can be obtained in our furnaces, or that the materials of our furnaces and vessels can support.

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The hope of attaining this desirable and important object gave rise to the experiments which I have now the honour of communicating. How far I may have succeeded, or whether the means employed were adequate to the end proposed, is, with all deference, submitted to this illustrious Society.

This attempt is founded upon the construction and application of an intermediate measure, which takes in both the heats that are measurable by the mercurial thermometer, and a sufficient number of those that come within the province of mine to connect the two together; the manner of doing which will be apparent from the three first figures (tab. XIV.); wherein F represents FAHRENHEIT's thermometer, with a continuation of the scale; W my thermometer; and M the intermediate measure divided into any number of equal parts at pleasure.

For if the heat of boiling water, or 212 degrees of FAHRENHEIT, be communicated to M, and its measure upon M marked, as at a ; and if the heat of boiling mercury, or 600° of FAHRENHEIT, be also communicated to M, and marked as at b ; it is plain, that the number of degrees upon M between a and b will be equal to the interval between 212 and 600 , that is, to 388° upon FAHRENHEIT.

In like manner, upon exposing M to two different heats above ignition along with my thermometer pieces, if a certain degree of my scale be found to correspond with the point d , and another degree of mine with the point c ; then the interval between those two degrees upon mine must be equal to the interval dc ; and how many of FAHRENHEIT's that interval is equivalent to will be known from the preceding comparison. Thus we can find the number of FAHRENHEIT's degrees contained in any given extent of mine, and the degree of FAHRENHEIT's with which a given point of mine coincides; whence

whence either scale is easily reducible to the other through their whole range, whether we suppose FAHRENHEIT's continued upwards, or mine downwards.

For obtaining the intermediate thermometer different means were thought of; but the only principle which, upon attentive consideration, afforded any prospect of success, was the *expansion of metals*. This therefore was adopted, and among different methods of measuring that expansion, which either occurred to myself, or which I can find to have been practised by others, there is no one which promises either so great accuracy, or convenience in use, as a gage like that by which the thermometer pieces are measured: the utility of this gage had now been confirmed to me by experience, and the machines and long rods, which have been employed for measuring expansions on other occasions, were absolutely inadmissible here, on account of the insuperable difficulties of performing nice operations of this kind in a red heat, and of communicating a perfectly equal heat through any considerable extent.

To give a clearer idea of this species of gage, which, simple as it is, I am informed has been misunderstood by some of the readers of my former paper, a representation of one used on the present occasion is annexed in fig. 4. where ABCD is a smooth flat plate; and EF and GH two rulers or flat pieces, a quarter of an inch thick, fixed flat upon the plate, with the sides that are towards one another made perfectly true, a little further asunder at one end EG than at the other end FH; thus they include between them a long converging canal, which is divided on one side into a number of small equal parts, and which may be considered as performing the offices both of the tube and scale of the common thermometer. It is obvious, that if a body, so adjusted as to fit exactly at the wider end of
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this canal, be afterwards diminished in its bulk by fire, as the thermometer pieces are, it will then pass further in the canal, and more and more so according as the diminution is greater; and conversely, that if a body, so adjusted as to pass on to the narrow end, be afterwards expanded by fire, as is the case with metals, and applied in that expanded state to the scale, it will not pass so far; and that the divisions on the side will be the measures of the expansions of the one, as of the contractions of the other, reckoning in both cases from that point to which the body was adjusted at first.

I is the body whose alteration of bulk is thus to be measured, which, in the present instance, is a piece of fine silver: this is to be gently pushed or slid along, towards the end FH, till it is stopped by the converging sides of the canal.

K is a little vessel formed in the gage for this particular series of experiments, the use of which will appear hereafter.

The *contraction*, which the thermometer pieces receive from fire, is a permanent effect, not variable by an abatement of the heat, and which accordingly is measured commodiously and at leisure, when the pieces are grown cold. But the *expansion* of bodies is only temporary, continuing no longer than the heat does that produced it; and therefore its quantity, at any particular degree of heat, must be measured in the moment while that heat subsists. And further, if the heated piece was applied to the cold gage, the piece would be deprived of a part of its heat on the first contact; and as the gage receives some degree of expansion from heat as well as the piece, it is plain that in this case the piece would be diminished in its bulk, and the gage enlarged, before the measurement could be taken. It is therefore necessary that both of them be heated to an exact equality; and in that state we can measure, not indeed the *true expansion of*

either, but the *excess* of the expansion of one above that of the other, which is sufficient for the present purpose, as we want only an uniform and graduated effect of fire, and it is totally immaterial whether that effect be the absolute expansion of one or the other body, or the difference of the two, provided only that its *quantity* be sufficient to admit of nice measurement.

Some difficulties occurred with respect to the choice of a proper *matter* for the gage; the essential requisites of which are, to have but little expansibility, and to bear the necessary fires without injury. All the metals, except gold and silver, would calcine in the fire: those two are indeed free from that objection, and accordingly it is of the most expansible of them that the piece is made; but if the gage also was made of the same, the measure itself would expand just as much as the body to be measured, and no expansion at all would be sensible; and though the gage was made of one of those metals, and the piece of the other, the difference between their expansions would be too small to give any satisfactory results, as more than two-thirds of the real expansion of either would be lost or taken off by the other.

For these reasons I had recourse to earthy compositions, which expand by heat much less than metallic bodies, and bear the necessary degrees of fire without the least injury. I made choice of tobacco-pipe clay, mixed with charcoal in fine powder, in the proportion of three parts of the charcoal to five of the clay by weight. By a free access of air, in the burning by which the gage is prepared for use, the charcoal is consumed, and leaves the clay extremely light and porous; from which circumstance it bears sudden alternations of cold and heat, often requisite in these operations, much better than the clay alone. Another and more important motive for the use of charcoal

was, that in consequence of the remarkable porosity which it produces in the clay, it would probably diminish the expansibility, by occasioning the mass to contain, under an equal surface, a much less quantity of solid or expansible matter. It may be objected to this idea, that the expansions of metals, in Mr. ELLICOTT's * and Mr. SMEATON's † experiments, do not appear to have any connection at all with their densities: but the cases are by no means parallel; for there the comparison lies between different species of matter; but here, between one and the same matter in different states of compactness. If a metal could be treated as clay is in this instance, that is, if a large bulk of any foreign matter could be blended with it, and this matter afterwards burnt out, so as to leave the metallic particles at the same distances to which they had been separated by the mixture of it, we may presume that the metal thus enlarged would not expand so much as an equal volume of the solid metal. Such at least were the ideas which determined my choice to a composition of clay and charcoal powder; and being afterwards desirous of satisfying myself whether they had any foundation in fact, I have, since the experiments were made, prepared some pieces of clay with and without charcoal, and having burnt them in the same fire, I ground them at the sides, to make them both fit exactly to the same division near the narrow end of the gage; then, examining their expansions by equal heats, I found the piece with charcoal to expand only one-third part so much as that without; and thus was fully satisfied with the composition of the gage.

To ascertain a fixed point on the scale for the divisions to be counted from, the silver piece and gage were laid together for

* Phil. Transact. vol. XLVII. p. 485.

† Ibid. vol. XLVIII. p. 612.

some time in spring water, of the temperature of 50° of FAHRENHEIT: the point which the piece went to in this cold state is that marked 0 near the narrow end of the gage. The adjustment is re-examined at the beginning and end of every succeeding experiment, lest the repeated attrition, in sliding the piece backwards and forwards, should wear off so much from the surface of this soft metal as to occasion an error in the minute quantities here measured.

The apparatus is then exposed successively to different degrees of heat, with the piece lying always in a part of the canal at least as wide as it is expected to fill when expanded, otherwise the sides of the gage would be burst asunder by its expansion, as I experienced in some of my first trials. When the whole has received any particular degree of heat desired, the piece is cautiously and equably pushed along, till it is stopped by the convergency of the sides, of which I always find notice given me by the gage itself (which is small and light) beginning to move upon the continuance of the impulse. A flat slip of iron, a little narrower than the piece, bent down to a right-angle at one end, and fixed in a long handle at the other, makes a convenient instrument for pushing the piece forward, or drawing it back again, whilst red-hot: this instrument, at every time of using, is heated to the same degree as the piece itself.

The heat of boiling water is taken without difficulty, by keeping the apparatus in boiling water itself during a sufficient space of time for the full heat to be communicated to it. The water I made use of was a very fine spring water, which on chemical trials appeared very nearly equal in purity to that of rain or snow; and I had previously satisfied myself, by trials in the cold, that the gage and piece being wet, or under water, made no difference in the measurement. The expansion of the

silver by this heat, that is, by an increase of the heat from 50° to 212° , or a period containing 162° of FAHRENHEIT, was just 8° of the gage or intermediate thermometer M; whence one of these degrees, according to this experiment, contains just $20^{\circ}\frac{1}{4}$ of FAHRENHEIT'S. The operation was many times repeated, and the result was always precisely the same.

For the boiling heat of mercury, it was necessary to proceed in a different manner; not to convey the heat from the mercury to the instrument, but to convey it equally to them both from another body. I made a small vessel for holding the mercury in the gage itself, seen at K fig. 4. and more distinctly in fig. 5. which is a transverse section of the gage through this vessel. The plate CD, which forms the bottom of the canal, serves also for the bottom of the vessel, which is situated close to the side of the canal, and as near as could be to that part of it, in which both the silver piece, and the divisions required for this particular experiment, are contained. By this arrangement it is presumed, that all the parts concerned in the operation will receive very nearly an equal heat.

The gage, with some mercury in the vessel, was laid upon a smooth and level bed of sand, on the bottom of an iron muffle kept open at one end; the fire increased very gradually till the mercury boiled, and then continued steady, so as just to keep it boiling, for a considerable time. The boiling heat of mercury was thus found to be $27^{\circ}\frac{1}{2}$ of the intermediate thermometer, which answering to an interval of 550° of FAHRENHEIT, makes one degree of this equal to just 20° of his; a result corresponding even beyond my expectations with that which boiling water had given.

These standard heats of FAHRENHEIT'S thermometer are obtained with little difficulty on a common fire; but it is far otherwise

otherwise with the higher ones in which mine begins to apply ; and all the precautions I could take, by using a close muffle, surrounding it as equally as possible with the fuel, varying its position with respect to the draught of air, &c. proved insufficient for securing the necessary equality of heat even through the small space concerned in these experiments. Nor had I any idea, before the discovery of this thermometer, of the extreme difficulty, not to say impracticability, of obtaining, in common fires, or in common furnaces, an uniform heat through the extent even of a few inches. Incredible as this may appear at first sight, whoever will follow me in the operations I have gone through, placing accurate measures of the heat in different parts of one and the same vessel, will soon be convinced of its truth, and that he can no otherwise expect to communicate with certainty an equal heat to different pieces, than by using a fire of such magnitude as to exceed perhaps some hundreds of times the bulk of the matters required to be heated.

To such large body of fire, therefore, after many fruitless attempts in small furnaces, not a little discouraging by the irregularity of their results, I at length had recourse, fitting up for this purpose an iron oven, used for the burning-on of enamel colours upon earthen ware, about four feet long, by two and a half wide, and three feet high, which is heated by the flame of wood conducted all round it. An iron muffle, four inches wide, two inches and three quarters high, and ten inches long, containing the gage and piece, was placed in the middle of this oven, and the vacancy between them filled up with earthen ware, to increase the quantity of ignited matter, and thereby communicate the heat more equably from the oven to the muffle. In such a situation of the muffle, in the
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center of an oven more than five hundred times its own capacity, it could not well fail of being heated pretty uniformly, at least through the small space which these experiments required; nor have I found any reason to suspect that it was not so.

The gage being laid flat upon the bottom of the muffle, with the silver piece in the canal as before, some of the clay thermometer pieces were set on end upon the silver piece, with that end of each downwards which is marked to go foremost in measuring it; that is, they were in contact with the silver in that part of their surface by which their measure is afterwards ascertained. I was led to this precaution by an experiment I had made upon another occasion, in which a number of thermometer pieces having been set upright upon an earthen-ware plate, over a small fire, till the plate became red-hot, all the pieces were found diminished, some of them more than two degrees, at the lower ends which rested upon the plate, whilst the upper ends were as much enlarged, not having yet passed the stage of extension which, as observed in the former paper, always precedes the thermometric diminution: thus we see how punctually every part of the piece obeys the heat that acts upon it.

The fire about the oven was slowly increased for some hours, and kept as even and steady as possible, by an experienced fireman, under my own inspection. Upon opening a small door, which had been made for introducing the apparatus, and looking in from time to time, it was observed, that the muffle, with the adjacent parts of the oven and ware, acquired a visible redness at the same time; and in the progress of the operation, the eye could not distinguish the least dissimilarity in the aspect of the different parts; whereas in small fires, the difference not only between the two ends of the muffle, but in much less distances, is such as to strike the eye at once.

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When the muffle appeared of a low red heat, such as was judged to come fully within the province of my thermometer, it was drawn forward, towards the door of the oven; and its own door being then nimbly opened by an assistant, I immediately pushed the silver piece as far as it would go. But as the division which it went to could not be distinguished in that ignited state, the muffle was lifted out, by means of an iron rod passed through two rings made for that purpose, with care to keep it steady, and avoid any shake that might endanger the displacing of the silver piece.

When grown sufficiently cold to be examined, I noted the degree of expansion which the silver piece stood at, and the degree of heat shewn by the thermometer pieces measured in their own gage; then returned the whole into the oven as before, and repeated the operation with a stronger heat, to obtain another point of correspondence on the two scales.

The first was at $2^{\circ}\frac{1}{4}$ of my thermometer, which coincided with 66° of the intermediate one; and as each of these last has been before found to contain 20 of FAHRENHEIT's, the 66 will contain 1320; to which add 50, the degree of his scale to which the 0 of the intermediate thermometer was adjusted, and the sum, 1370, will be the degree of FAHRENHEIT's corresponding to my $2^{\circ}\frac{1}{4}$.

The second point of coincidence was at $6^{\circ}\frac{1}{4}$ of mine, and 92° of the intermediate; which 92 being, according to the above proportion, equivalent to 1840 of FAHRENHEIT, add 50 as before to this number, and my $6^{\circ}\frac{1}{4}$ is found to fall upon the 1890th degree of FAHRENHEIT.

It appears from hence, that an interval of 4 degrees upon mine is equivalent to an interval of 520° upon his; consequently 1 of mine to 130 degrees of his; and that the 0 of mine corresponds

to his $1077^{\circ}\frac{1}{2}$. Several other trials were made, which gave results so nearly alike, that I have little apprehension of any material error.

From these data it is easy to reduce either scale to the other through their whole range; and from such reduction it will appear, that an interval of near 480° remains between them, which the intermediate thermometer serves as a measure for; that mine includes an extent of about 32000 of FAHRENHEIT's degrees, or about 54 times as much as that between the freezing and boiling points of mercury, by which mercurial ones are naturally limited; that if the scale of mine be produced downwards, in the same manner as we have supposed FAHRENHEIT's to be produced upwards, for an ideal standard, the freezing point of water would fall nearly on 8° below 0 of mine, and the freezing point of mercury a little below $8^{\circ}\frac{1}{2}$; and that, therefore, of the extent of now measurable heat, there are about $\frac{1}{10}$ ths of a degree of my scale from the freezing of mercury to the freezing of water; 8° from the freezing of water to full ignition; and 160° above this to the highest degree I have hitherto attained.

As we are now enabled to compare not only the higher degrees among themselves, and the lower among themselves, upon their respective scales, but likewise the higher and lower with each other in every stage, it may be proper to take a general view of the whole range of measurable heat, as expressed both in FAHRENHEIT's denominations and in mine; and for this purpose I have drawn up a little table of a few of the principal points that have been ascertained, to shew their mutual relations or proportions to each other: any other points that have been, or hereafter may be, observed, by these or any other known thermometers, may be inserted at pleasure.

	FAHR.	WEDG.
Extremity of the scale of my thermometer	32277°	240°
Greatest heat of my small air-furnace -	21877	160
Cast iron melts - - - -	17977	130
Greatest heat of a common smith's forge	17327	125
Welding heat of iron, greatest -	13427	95
least - - - -	12777	90
Fine gold melts - - - -	5237	32
Fine silver melts - - - -	4717	28
Swedish copper melts - - -	4587	27
Brass melts - - - -	3807	21
Heat by which my enamel colours are burnt on	1857	6
Red-heat fully visible in day-light -	1077	0
Red-heat fully visible in the dark -	947	1
Mercury boils - - - -	600	3 $\frac{623}{1000}$
Water boils - - - -	212	6 $\frac{658}{1000}$
Vital heat - - - -	97	7 $\frac{542}{1000}$
Water freezes - - - -	32	8 $\frac{42}{1000}$
Proof spirit freezes - - - -	0	8 $\frac{289}{1000}$
The point at which mercury congeals, consequently the limit of mercurial thermometers, - - - -	} about 40	8 $\frac{596}{1000}$

To assist our conceptions of this subject, it may be proper to view it in another light, and endeavour to present it to the eye; for *numbers*, on a high scale, are with difficulty estimated and compared by the mind. I have therefore completed the scales of which a part is represented in fig. 1. and 3. by continuing the same equal divisions, both upwards and downwards, as far as the utmost limits of heat that have hitherto been attained and measured*.

* Mr. WEDGWOOD presented this, in the form of a very long roll, to the Society.

In a scale of heat drawn up in this manner, the comparative extents of the different departments of this grand and universal agent are rendered conspicuous at a single glance of the eye. We see at once, for instance, how small a portion of it is concerned in animal and vegetable life, and in the ordinary operations of nature. From freezing to vital heat is barely a five-hundredth part of the scale; a quantity so inconsiderable, relatively to the whole, that in the higher stages of ignition, ten times as much might be added or taken away, without the least difference being discernible in any of the appearances from which the intensity of fire has hitherto been judged of. From hence, at the same time, we may be convinced of the utility and importance of a physical measure for these higher degrees of heat, and the utter insufficiency of the common means of discriminating and estimating their force. I have too often found differences, astonishing when considered as a part of this scale, in the heats of my own kilns and ovens, without being perceivable by the workmen at the time, or till the ware was taken out of the kiln.

SINCE the foregoing experiments were made, I have seen a very curious Memoir by Mess. LAVOISIER and DE LA PLACE, containing a method of measuring heat by the quantity of ice which the heated body is capable of liquefying. The application of this important discovery, as an intermediate standard measure between FAHRENHEIT's thermometer and mine, could not escape me, and I immediately set about preparing an apparatus, and making the experiments necessary for that purpose;

pose; in hopes either of attaining by this method a greater degree of accuracy than I could expect from any other means, or of having what I had already done confirmed by a series of experiments upon a different principle.

But in the prosecution of these experiments I have, to my great mortification, hitherto failed of success; and I should have contented myself for the present with saying little more than this, if some phænomena had not occurred, which appear to me not unworthy of farther investigation.

The authors observe, that if ice, cooled to whatever degree below the freezing point, be exposed to a warmer atmosphere, it will be brought up to the freezing point through its whole mass before any part of its surface begins to liquefy; and that consequently ice, beginning to melt on the surface, will be always exactly of the same temperature, *viz.* at the freezing point; and that if a heated body be inclosed in a hollow sphere of such ice, the whole of its heat will be taken up in liquefying the ice; so that if the ice be defended from external warmth, by surrounding it with other ice in a separate vessel, the weight of the water produced from it will be exactly proportional to the heat which the heated body has lost; or, in other words, will be a true physical measure of the heat.

For applying these principles in practice, they employ a tin vessel, divided, by upright concentric partitions, into three compartments, one within another. The innermost compartment is a wire cage, for receiving the heated body. The second, surrounding this cage, is filled with pounded ice, to be melted by the heat; and the outermost is filled also with pounded ice, to defend the former from the warmth of the atmosphere. The first of these ice compartments terminates at bottom in a stem like a funnel, through which the water is conveyed off; and the other ice compartment terminates in a separate canal, for

discharging the water into which *that* ice is reduced. As soon as the heated body is dropped into the cage, a cover is put on, which goes over both that and the first ice compartment; which cover is itself a kind of shallow vessel, filled with pounded ice, with holes in the bottom for permitting the water from this ice to pass into the second compartment, all the liquefaction that happens here, as well as there, being the effect of the heated body only. Over the whole is placed another cover with pounded ice, as a defence from external warmth.

As soon as this discovery came to my knowledge, on the 23d of February, a thaw having begun three days before, after a frost which had continued with very little intermission from the 24th of December, I collected a quantity of ice, and stored it up in a large cask in a cellar.

I thought it necessary to satisfy myself in the first place, by actual experiment, that ice, how cold soever it may be, comes up to the freezing point through its whole mass before it begins to liquefy on the surface. For this purpose I cooled a large fragment of ice, by a freezing mixture, to 17° of FAHRENHEIT's thermometer, and then hung it up in a room whose temperature was 50° . When it began to drop, it was broken, and some of the internal part nimbly pounded and applied to the bulb of a thermometer that was cooled by a freezing mixture below 30° . The thermometer rose to, and continued at, 32° ; being then taken out, and raised by warmth to 40° , some more of the same ice, applied as before to the bulb, sunk it again to 32° ; so that no doubt could remain on this subject.

Apprehensive that pounded ice, directed by the authors, might imbibe and retain more or less of the water by capillary attraction, according to circumstances, and thereby occasion some error in the results, I thought it necessary to satisfy myself in this respect
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also by experiment. I therefore pounded some ice, and laid it in a conical heap on a plate; and having at hand some water, coloured with cochineal, I poured it gently into the plate, at some distance from the heap: as soon as it came in contact with the ice, it rose hastily up to the top; and on lifting up the lump, I found that it held the water, so taken up, as a sponge does, and did not drop any part of it till the heat of my hand, as I suppose, began to liquefy the mass. On further trials I found, that in pounded ice pressed into a conical heap, the coloured water rose, in the space of three minutes, to the height of two inches and a half; and by weighing the water employed, and what remained upon the plate unabsorbed, it appeared, that four ounces of ice had thus taken up, and retained, one ounce of water.

To further ascertain this absorbing power, in different circumstances, more analogous to those of the process itself, I pressed six ounces of pounded ice pretty hard into the funnel, having first introduced a wooden core in order to leave a proper cavity in the middle: then, taking out the core, and pouring an ounce of water upon the ice, I left the whole for half an hour; at the end of which time the quantity that ran off was only 12 pennyweights and 4 grains, so that the ice had retained 7 pennyweights and 20 grains, which is nearly one-twelfth of its own weight, and two-fifths of the weight of the water.

These previous trials determined me, instead of using pounded ice, to fill a proper vessel with a solid mass of ice, by means of a freezing mixture, as the frost was now gone, and then expose it to the atmosphere till the surface began to liquefy. The apparatus I fitted up for this purpose was made of earthen ware well glazed, and is represented in fig. 6. (tab. XV.).

A, is a large funnel, filled with a solid mass of ice. B, a cavity in the middle of this ice, formed, part of the way, by scraping with a knife, and for the remaining part, by boring with a hot iron wire. C, one of my thermometer pieces, which serves for the heated body, and rests upon a coil of brass wire: it had previously been burnt with strong fire, that there might be no danger of its suffering any further diminution of its bulk by being heated again for these experiments. D, a cork stopper in the orifice of the funnel. E, the exterior vessel, having the space between its sides and the included funnel A, filled with pounded ice, as a defence to the ice in the funnel. F, a cover for this exterior vessel, filled with pounded ice for the same purpose. G, a cover for the funnel, filled also with pounded ice, with perforations in the bottom for allowing the water from this ice to pass down into the funnel.

The thermometer piece was heated in boiling water, taken up with a pair of small tongs equally heated, dropped instantly into the cavity B, and the covers put on as expeditiously as possible; the bottom of the funnel being previously corked, that the water might be detained till it should part with all its heat, and likewise to prevent the water from the other ice, which ran down on the outside of the funnel, from mingling with it.

After standing about ten minutes, the funnel was taken out, wiped dry, and uncorked over a weighed cup: the water that ran out weighed 22 grains. Thinking this quantity too small, as the piece weighed 72 grains, I repeated the experiment, and kept the piece longer in the funnel; but the water this time weighed only 12 grains. Being much dissatisfied with this result, I made a third trial, continuing the piece much longer in the cavity; but the quantity of water was now still less, not

amounting to quite three drops; and, to my great surprise, I found the piece frozen to the ice, so as not to be easily got off, though all the ice employed was, at the beginning of the experiment, in a thawing state.

I had prepared the apparatus for taking the boiling heat of mercury; but being entirely discouraged by these very unequal results, I gave that up, for the present at least, and heating the piece to 6° of my thermometer, turned it nimbly out of the case in which it was heated into the cavity, throwing some fragments of ice over it. In about half an hour, I drew off the water, which amounted to 11 pennyweights; then stopping the funnel again, and replacing the covers, I left the whole about seven hours.

At the end of that time, I found a considerable quantity of water in the funnel: the melting of the ice had produced a cavity between it and the sides, great part of the way down, which, as well as that in the middle, was nearly full. The water nevertheless ran out so slowly, that I apprehended something had stopped the narrow end of the funnel, but the true cause became afterwards apparent upon examining the state of the ice. The fragments which I had thrown over the thermometer piece were frozen entirely together, and in such a form as they could not have assumed without fresh water superadded and frozen upon them, for the cavities between them were partly filled with new ice. I endeavoured to take the ice out with my fingers, but in vain; and it was with some difficulty I could force it asunder even with a pointed knife, to get at the thermometer piece. When that was got out, great part of the coiled wire was found enveloped in new ice. The passage through the ice to the stem of the funnel, which I had made pretty wide with a thick iron wire red-hot, was so nearly closed
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up, that the flow draining off of the water was now sufficiently accounted for, and indeed this draining was the only apparent mark of any passage at all. On taking the ice out of the funnel, and breaking it to examine this canal, I found it almost entirely filled up with ice projecting from the solid mass in crystalline forms, similar in appearance to the crystals we often meet with in the cavities of flints and quartzose stones.

If, after all these circumstances, any doubt could have remained of the ice in question being a new production, a fact which I now observed must have removed all suspicion. I found a coating of ice, of considerable extent and perfectly transparent, about a tenth of an inch in thickness, upon the outside of the funnel, and on a part of it which was not in contact with the surrounding ice, for that was melted to the distance of an inch from it.

Some of the ice being scraped off from the inside of the funnel, and applied to the bulb of the thermometer, the mercury sunk from 50° to 32° , and continued at that point till the ice was melted; after which, the water being poured off, it rose in a little time to 47° .

Astonished at these appearances, of the water freezing after it had been melted, though surrounded with ice in a melting state, and in an atmosphere about 50° , where no part of the apparatus or materials could be supposed to be lower than the freezing point, I suspected at first that some of the salt of the freezing mixture might have got into the water, and that this, in dissolving, might perhaps absorb, from the parts contiguous to it, a greater proportion of heat than the ice of pure water does. But the water betrayed nothing saline to the taste, and I had applied the freezing mixture with my own hands with great care, to prevent any of it being mixed with the water.

To remove all doubts, however, upon this point, I purposed repeating the experiment with some pieces of the ice I had stored up in the cellar, to see if this would congeal, after thawing, in the same manner. But going to fetch the ice, and examining it in the cask in which it was kept, I was perfectly satisfied with the appearances I found there; for though much of it was melted, yet the fragments were frozen together, so that it was with difficulty I could break or get out any pieces of it with an iron spade; and, when so broken, it had the appearance of *breccia* marble or plum-pudding stone, for the fragments had been broken and rammed into the cask with an iron mall.

A porcelain cup being laid upon some of this ice about half an hour, in a room whose temperature was 50° , it was found pretty firmly adhering, and when pulled off, the ice exhibited an exact impression of the fluted part of the cup which it had been in contact with; so that the ice must necessarily have liquefied first; and afterwards congealed again. This was repeated several times, with the same event. Fragments of the ice were likewise applied to one another, to sponges, to pieces of flannel and of linen cloth, both moist and dry: all these, in a few seconds, began to cohere, and in about a minute were frozen so as to require some force to separate them. After standing an hour, the cohesion was so firm, that on pulling away the fragments of ice from the woollen and sponge, they tore off with them that part of the surface which they were in contact with, though at the same time both the sponge and flannel were filled with water which that very ice had produced.

To make some estimate of the force of the congelation, which was stronger on the two bodies last mentioned than on

linen, I applied a piece of ice to a piece of dry flannel which weighed two pennyweights and a half, and surrounded them with other ice. After lying together three quarters of an hour, taking the piece of ice in my hand and hooking the flannel to a scale, I found a weight of five ounces to be necessary for pulling it off, and yet so much of the ice had liquefied as to increase the weight of the flannel above 12 pennyweights. I then weighed the piece of ice, put them together again, and four hours after found them frozen so firmly as to require 78 ounces for their separation, although, from 42 pennyweights of the ice, 15 more had melted off: the surface of contact was at this time nearly a square inch. I continued them again together for seven hours; but they now bore only 62 ounces, the ice being diminished to 14 pennyweights, and the surface of contact reduced to about six-tenths of a square inch.

Having seen before that pounded ice absorbs water in very considerable quantity, I suspected that something of the same kind might take place even with entire masses; and experiment soon convinced me, that even apparently solid pieces of ice will imbibe water, slower or quicker according to its stage of decay. I have repeatedly heated some of my thermometer pieces, and laid them upon ice, in which they made cavities of considerable depth, but the water was always absorbed, sometimes as fast as it was produced, leaving both the piece and the cavity dry.

Thus, though I cannot sufficiently express how much I admire the discovery that gave rise to these experiments, I have nevertheless to lament my not being able to avail myself of it at present for the purpose I wished to apply it to.

That in my experiments the two seemingly opposite processes of nature, congelation and liquefaction, went on together, at

the same instant, in the same vessel, and even in the same fragment of ice, is a fact of which I have the fullest evidence that my senses can give me; and I shall take the liberty of suggesting a few hints, which may tend perhaps to elucidate their cause, and to shew that they are not so incompatible as at first sight they appear to be.

It occurred to me at first, that water highly attenuated and divided, as when reduced into vapour, may freeze with a less degree of cold than water in its aggregate or grosser form; hence hoar-frost is observed upon grass, trees, &c. at times when there is no appearance of ice upon water, and when the thermometer is above the freezing point*. BOERHAAVE, I find, in his elaborate theory of fire, assigns 33° as the freezing point of vapour, and even of water when divided only by being imbibed in a linen cloth.

* I am aware, that experiments and observations of this kind are not fully decisive; that the atmosphere may, in certain circumstances, be much warmer or colder than the earth and waters, which, in virtue of their density, are far more retentive of the temperature they have once received, and less susceptible of transient impressions; that even insensible undulations of water, from the slightest motion of the air, by bringing up warmer surfaces from below, may prove a further impediment to the freezing; and, therefore, that the degree of cold, which is sufficient to produce hoar-frost, may possibly, if continued long enough, be sufficient also to produce ice. I am not acquainted with any satisfactory experiments or observations yet made upon the subject; nor do I advance the principle as a certain, but as a probable one, which occurred to me at the moment, which is countenanced by general observation, and consistent to many known facts; for there are numerous instances of bodies, in an extreme state of division, yielding easily to chemical agents which, before such division, they entirely resist: thus some precipitates, in the very subtle state in which they are at first extricated from their dissolvents, are re-dissolved by other menstrua, which, after their concretion into sensible molecules, have no action upon them at all.

Now, as the atmosphere abounds with watery vapour, or water dissolved and chemically combined, and must be particularly loaded with it in the neighbourhood of melting ice; as the heated body introduced into the funnel must necessarily convert a portion of the ice or water there into vapour; and as ice is known to melt as soon as the heat begins to exceed 32° , or nearly one degree lower than the freezing point of vapour; I think we may from hence deduce, pretty satisfactorily, all the phænomena I have observed. For it naturally follows from these principles, that vapour may freeze where ice is melting; that the vapour may congeal even upon the surface of the melting ice itself; and that the heat which (agreeably to the ingenious theory of Dr. BLACK) it emits in freezing, may contribute to the further liquefaction of that very ice upon which the new congelation is formed.

I would further observe, that the freezing of water is attended with plentiful evaporation in a close as well as an open vessel, the vapour in the former condensing into drops on the under side of the cover, which either continue in the form of water, or assume that of ice or a kind of snow, according to circumstances*; which evaporation may perhaps be attributed to the heat that was combined with the water, at this moment rapidly making its escape, and carrying part of the aqueous fluid off with it. We are hence furnished with a fresh and continual source of vapour as well as of heat; so that the processes of liquefaction and congelation may go on uninterruptedly together, and even necessarily accompany one another, although, as the freezing must be in an under proportion to the melting, the whole of the ice must ultimately be consumed.

* See Mr. BARON's paper on this subject, in the Memoires of the Academy of Sciences at Paris for the year 1753.

In the remarkable instance of the coating of ice on the outside of the throat of the funnel, there are some other circumstances which it may be proper to take notice of. Neither the cover of the outer vessel, nor the aperture in its bottom which the stem of the funnel passed through, were air-tight, and the melting of the surrounding ice had left a vacancy of about an inch round that part of the funnel on which the crust had formed. As there was, therefore, a passage for air through the vessel, a circulation of it would probably take place: the cold and dense air in the vessel would descend into the rarer air of the room then about 50° , and be replaced by air from above. The effect of this circulation and sudden refrigeration of the air will be a condensation of part of the moisture it contains upon the bodies it is in contact with: the throat of the funnel, being one of those bodies, must receive its share; and the degree of cold in which the ice thaws being supposed sufficient for the freezing of this moist vapour, the contact, condensation, and freezing, may happen at the same instant.

The same principles apply to every instance of congelation that took place in these experiments; and a recollection of particulars which passed under my own eye convinces me, that the congelation was strongest in those circumstances where vapour was most abundant, and on those bodies which, from their natural or mechanic structure, were capacious of the greatest quantity of it; stronger, for instance, on sponge than on woollen, stronger on this than on the closer texture of linen, and far stronger on all these than on the compact surface of porcelain.

If, nevertheless, the principle I have assumed (that water highly attenuated will congeal with a less degree of cold than water in the mass) should not be admitted; another has above
been

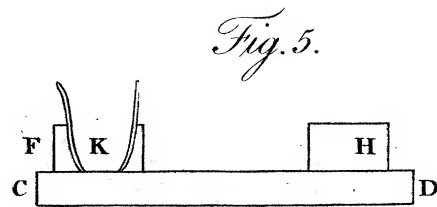
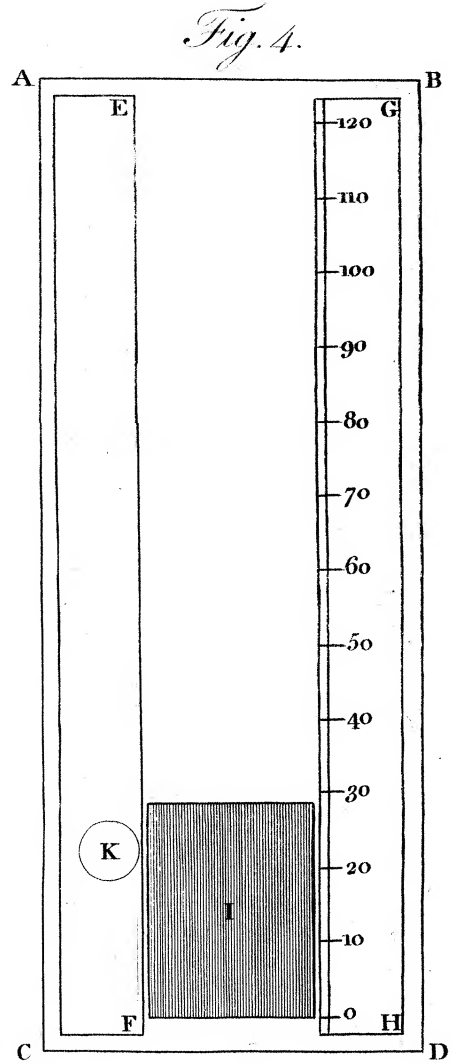
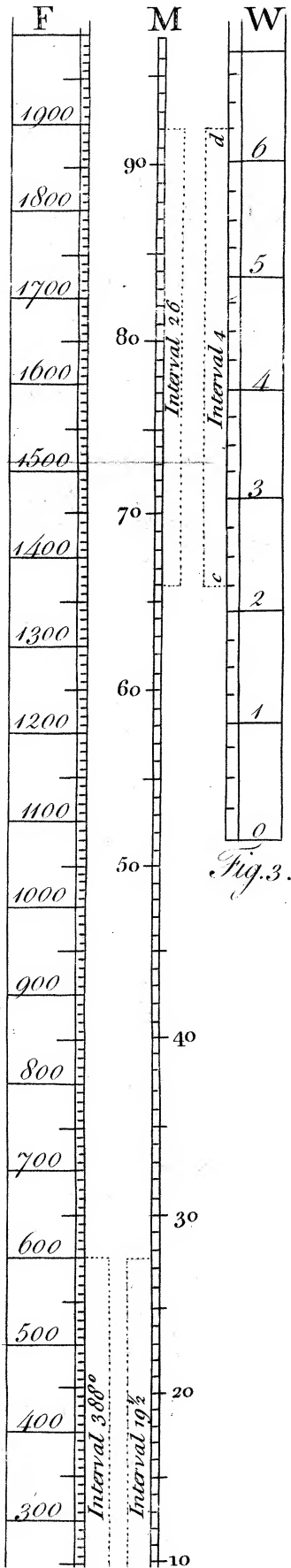
been hinted at, which experiments have decidedly established, from which the phænomena may perhaps be equally accounted for, and which, even though the other also is received, must be supposed to concur for some part of the effect; I mean, that *evaporation produces cold*; both vapour and steam carrying off some proportion of heat from the body which produces them. If, therefore, evaporation be made to take place upon the surface of ice, the contiguous ice will thereby be rendered colder; and as it is already at the freezing point, the smallest increase of cold will be sufficient for fresh congelation. It seems to be on this principle that the formation of ice is effected in the East Indies, by exposing water to a serene air, at the coldest season of the year, in shallow porous earthen vessels: part of the water transudes through the vessel, and evaporating from the outside, the remainder in the vessel becomes cold enough to freeze; the warmth of the earth being at the same time intercepted by the vessels being placed upon bodies little disposed to conduct heat*. If ice is thus producible in a climate where natural ice is never seen, we need not wonder that congelation should take place where the same principle operates amidst actual ice.

It has been observed above, that the heat emitted by the congealing vapour probably unites with and liquefies contiguous portions of ice; but whether the whole, either of the heat so emitted, or of that originally introduced into the funnel, is thus taken up; how often it may unite with other portions of ice, and be driven out from other new congelations; whether there exists any difference in its chemical affinity or

* See a description of this process in the Philosophical Transactions, vol. LXV. p. 253.

elective attraction to water in different states and the contiguous bodies; whether part of it may not ultimately escape, without performing the office expected from it upon the ice; and to what distance from the evaporating surface the refrigerating effect of the evaporation may extend; must be left for further experiments to determine.





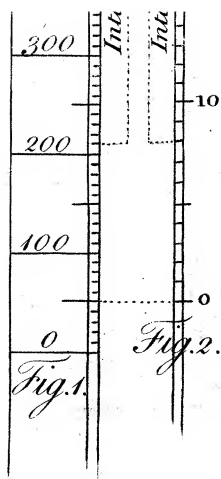


Fig. 6.

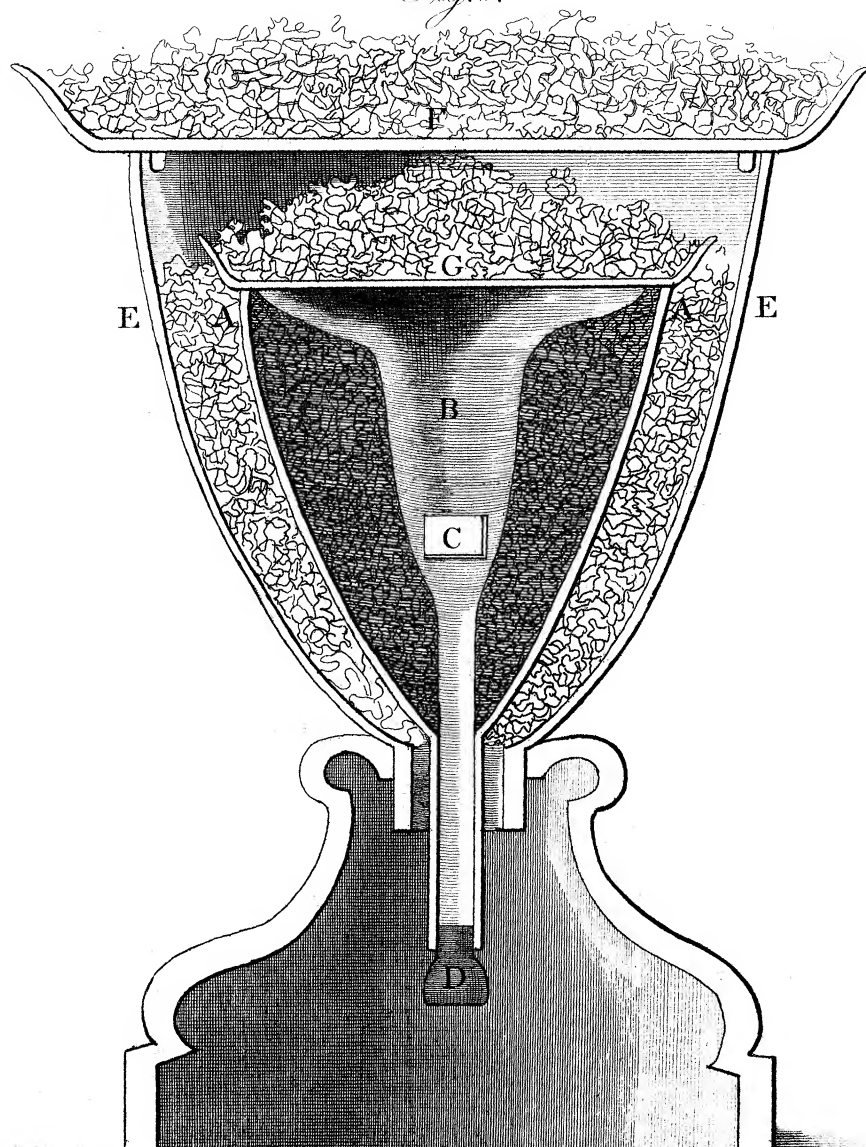


Fig. 1



Fig. 2

